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EDWIN DUNKIN, F.R.S., President, in the Chair.

John T. Stevenson, Auckland, New Zealand, was balloted for and duly elected a Fellow of the Society.

On the Necessary Distinction in Practical Astronomy between the True Mean Solar Day and the Mean Day adopted, from time to time, in the Construction of our Astronomical Tables, and in the Comparison of these Tables with Observations. By E. J. Stone, M.A., F.R.S.

The papers which have appeared in connection with this question in the *Monthly Notices* for 1883, December, and 1884, January, show that the physical conditions of the problem are most imperfectly understood. We certainly have not two independent mean Suns, and therefore two independent real Suns, and therefore two independent positions of the observer at any given time available in practical astronomy to satisfy any analytical requirements; nor can we measure motion correctly in terms of a time which is itself a fallible quantity, whose errors increase as the time increases; and it is equally impossible for us, so long as we are ignorant of the value of the Sun's mean motion in longitude in the true mean solar day, to determine times from the motion of the meridian *alone* expressed in terms of a unit whose length itself depends upon the *relative* motions of the meridian over that of the Sun in mean longitude; for the two motions are perfectly independent. We have no doubt attempted to do this; and theoretical astronomers have supposed that we were really using the *true* mean solar day as our unit; and it is because we have attempted to do what is physically impossible, and have directly introduced into our

determinations of time important conditional equations which would be true if we *were* using the true mean solar day as our unit, but which are not true in practice, that we have introduced discordances between our theories and observations which have led to the necessity of such hypotheses as "changes in the time of the rotation of the Earth on its axis," or "unknown physical actions," and have wasted the energies of some of our best men and squandered thousands of pounds.

It is absolutely impossible for us to determine motions in terms of a variable t which is not quantitatively referred to some continuous motion which is itself measurable, or to some such difference between motions; and the equation which connects the standard motion and the variable t, and from which t is determined, is, of necessity, subject to the conditions of its use in practice, *an exact equation*. It is this equation, subject to its use in practice, and not any theoretical views of our own, which fixes the scale upon which our times are measured, or the unit of time which is adopted in practical astronomy. But the observer's position depends upon two perfectly independent motions—the motion of the Earth about its centre of gravity and the motion of that centre of gravity itself. If the symbols ω and n denote the motion of the Earth about its axis, and the mean motion of the centre of gravity of the Earth in longitude respectively, then, before we can compare our theories of the motions of the centres of gravity of the Earth, Moon, and planets with observations, or determine the constants involved in our theoretical expressions, we must establish some relationship between ω and n; but, since the motions of ω and n are perfectly independent of each other, we can only establish such a relationship indirectly through T, some interval of time which we adopt as our unit. With every variation of T, ω and n will both vary, but so long as the symbols represent the physical quantities which I have indicated, and when they no longer indicate these physical quantities, we have no concern with them, the variations must satisfy the equations

$$(1)\quad \frac{\delta\omega}{\omega} = \frac{\delta n}{n} = \frac{\delta \mathrm{T}}{\mathrm{T}}.$$

If we will introduce in our practical determinations of mean times any conditional equations depending upon the length of the *true* mean solar day, or any other similar physical conditions, we must employ an *exact equation* between ω, n, t, or we shall necessarily introduce secular separations between the two motions which fix the observer's position; but if t denote the length of the true mean solar day expressed in terms of any unit T, we have identically, from the definition of the mean solar day,

$$(2)\quad \omega = \frac{360 \times 60 \times 60}{t} + n.$$

And if the symbol T_0 denote the length of the true mean solar day, we shall have

$$t \,.\, T = T_0,$$

and therefore

$$(3)\quad \omega = 360 \times 60 \times 60 \times \frac{T}{T_0} + n.$$

This equation (3) is true for all corresponding values of ω, T, and n, and is exact, but differentiating we have

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta T}{T_0} + \delta n,$$

or since

$$\frac{\delta T}{T} = \frac{\delta n}{n},$$

we have, if $T = T_0$ nearly,

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta n}{n} + \delta n.$$

This equation establishes the only possible law of variation between ω and n when ω and n denote the physical quantities of our problem. If, therefore, n_0 is the true value of the mean motion in longitude of the centre of gravity of the Earth in the true mean solar day T_0, we have

$$\omega_0 = 360 \times 60 \times 60 + n_0\,;$$

but, when we adopt n instead of n_0, the true expression is not

$$\omega = 360 \times 60 \times 60 + n,$$

as astronomers have hitherto assumed, but

$$\omega = 360 \times 60 \times 60 + n_0 + 360 \times 60 \times 60 \frac{n - n_0}{n_0} + n - n_0.$$

or

$$(4)\quad \omega = 360 \times 60 \times 60 + n + 360 \times 60 \times 60 \frac{n - n_0}{n_0},$$

This equation is exact. The omission of the term

$$360 \,.\, 60 \,.\, 60 \,.\, \frac{n - n_0}{n_0},$$

would at once, with times determined with the erroneous value of ω, introduce secular errors into the theoretical positions of the Sun, Moon, and planets. If, therefore, s denotes the sidereal time, we have

$$s = a + 24^h \,.\, t_0 + \frac{n_0}{15} \,.\, t_0,$$

if t_0 is the true mean solar time; but we have with equal exactness

$$s = a + \left(1 + \frac{n - n_0}{n^c}\right) . t + (n_0 + n - n_0) . t,$$

or

$$(5)\quad s = a + \left(24^h + n + 24^h \frac{n - n_0}{n_0}\right) . t,$$

when we adopt n for the Sun's mean motion. The only difference is that our time now is not *true mean solar time*, but mean time referred to some definite unit T.

If, therefore, we attempt from a given observed value of s to infer the true sidereal interval from mean noon by the formula usually adopted,

$$s = (a + nr),$$

where r is the number of complete days from epoch, we commit the error

$$-24^h . \frac{n - n_0}{n_0} . r.$$

But if we are using $n + \delta n$ instead of n we commit the error

$$-24^h \left(\frac{n + \delta n - n_0}{n_0}\right) . r.$$

The difference between these errors is a determinate quantity,

$$-24^h \frac{\delta n}{n_0} . r.$$

The question is, Can we neglect this difference? The answer is emphatically, No! I have proved this by a direct numerical calculation. It accounts for the existing large discordances between the lunar tables and observations, and such terms cannot therefore be neglected as insensible.

If we start from the given sidereal time of observation, as we do in practice, these results show that, instead of taking out our tabular places for the same noon before and after the change from n to $n + \delta n$, we must correct all our tabular places for an error of time

$$24^h \frac{\delta n}{n} . r,$$

in order that they may correspond to the new mean noon as they did correspond to the old mean noon. The difficulty which appears to be felt in this matter is probably due to overlooking the fact that if we commit such systematic errors in referring our sidereal times to our mean times, as I have proved we do commit so long as n_0 is unknown, we determine in practice not the true mean solar times, but other times; and we express, as the only times available to us, all our mean motions

in terms of these times, and not in terms of the true mean solar times as we have hitherto supposed. A change in the adopted value of n, therefore, necessitates a redetermination of all our mean motions, or a correction to the times such as I have applied; for when we determine our times directly from the motion of the meridian with any adopted value of ω, *all errors in our time determinations are thrown directly upon the theoretical expressions for the motions of the centres of gravity.*

This explains why Prof. Adams, in attempting to show that no errors have been made in these questions of tabular times, is driven to assert that we have nothing to do with the Right Ascension of the meridian at mean noon; and that my views must be wrong because the adopted tabular expressions for the motions of the centres of gravity are made to represent the same motions in the same absolute times before and after any arbitrary changes which we may be pleased to adopt in our time determinations. The way in which these small errors accumulate is no doubt difficult to get a firm grasp of. And perhaps the point may be put somewhat more simply as follows.

If we adopt a value n_1 for n, then the unit of time is certainly fixed and determinate. Let this value be T_1, and let ω_1 be the value of ω expressed in terms of T_1, and t_1 the length of the true mean solar day, then clearly

$$t_1 = \frac{360^\circ}{\omega_1 - n_1}.$$

And of n_2, T_2, ω_2, and t_2 are other corresponding values,

$$t_2 = \frac{360^\circ}{\omega_2 - n_2},$$

and

$$\frac{t_1}{t_2} = \frac{\omega_2 - n_2}{\omega_1 - n_1}.$$

But since these equations are exact, we have

$$\frac{\omega_2 - n_2}{\omega_1 - n_1} = \frac{T_2}{T_1} = \frac{n_2}{n_1}.$$

We have, therefore,

$$\frac{t_1}{t_2} = \frac{n_2}{n_1},$$

or

$$\frac{t_1 - t_2}{t_2} = \frac{n_2 - n_1}{n_1}.$$

It is, therefore, perfectly impossible for us to use the true mean solar day as our unit when we are continually changing the adopted values of n. But the question practically under consideration is, What will the errors of the false assumption amount to? The equation

$$\frac{t_1 - t_2}{t_2} = \frac{\delta n}{n}$$

answers the question. We shall make an error of

$$\frac{\delta n}{n} \times \text{days measured.}$$

The change thus made in 1864 accounts for the large increase of the discordances between Hansen's tables of the errors and observations, and the errors of the false assumption, therefore, cannot be neglected. But it may be said that astronomers have proved that the changes in the units of time due to changes from n to $n+\delta n$ in the adopted values of the Sun's mean motion in longitude are not in the ratio of $n : n+\delta n$, as assumed, but in the ratio of $\omega : \omega+\delta n$. Astronomers have certainly proved nothing of the kind, and it is perfectly impossible that any such result can be true when ω and n represent the physical quantities which I have made them represent. Prof. Cayley has proved that if we have two Suns—or two Earths, rather—moving with mean motions in longitude of n and $n+\delta n$ respectively in the adopted unit of time, then the mean solar days would differ by nearly

$$\frac{\delta n}{\omega} \times \text{length of a day.}$$

Prof. Cayley's result is of course perfectly correct on the assumptions made; but in practical astronomy the observer's position is fixed through the n and the ω, and n expresses the mean motion of the centre of gravity of the Earth in longitude. The n and $n+\delta n$, therefore, refer to the motion of the same physical point, and not to two independent moving points, as Prof. Cayley supposes in his problem. The result given by Prof. Cayley, therefore, does not prove that

$$\frac{\delta T}{T} \text{ is not } = \frac{\delta n}{n}$$

when the symbols have the physical meanings which I have assigned to them. As a matter of fact, Prof. Cayley obtains my result when restricted to a single moving point.

Prof. Adams has also shown that, subject to certain conditions,

$$\frac{\delta T}{T} \text{ is equal to } \frac{\delta n}{\omega}.$$

And this result is equally true on the assumptions made, but they are not those of the physical problem under consideration, as we can easily show. If we attempt to establish a relation between ω and n by successive approximations through ω, starting from $\omega = 360°$ in the sidereal day S, we can do this quite accurately provided we introduce no physical conditions which the symbols ω, n, and T are made to satisfy in our practical determinations of the time during the process of approximation. For, putting

$\omega = 360° + n$ and $360° + n + \delta n$ respectively, we have for the sidereal time respectively

$$s = \alpha + \left(24^h + \frac{n}{15}\right)t$$

and

$$s = \alpha + \left(24^h + \frac{n+\delta n}{15}\right).t^1$$

and therefore

$$= t^1 \left(1 + \frac{\delta n}{360° + n}\right).$$

But this result is utterly inconsistent with the assumption which is always made in practical astronomy that the true mean solar day is always used as our unit, and that we can therefore neglect the difference between t and t' in the large terms $24^h t$ and $24^h t'$, and thus write

$$24^h . t = 24^h . t'$$

whenever t' is an integer; this requires that

$$s = a + \frac{nt}{15} = a + \frac{(n+\delta n)t'}{15},$$

and therefore instead of

$$t = \left(1 + \frac{\delta n}{\omega}\right).t',$$

$$t = \left(1 + \frac{\delta n}{n}\right).t',$$

to satisfy the necessary conditions.

Prof. Adams's result is therefore not true, as we use the expressions

$$360 \times 60 \times 60 + n$$

and

$$360 \times 60 \times 60 + n + \delta n$$

in our practical determinations of time.

This point is rendered clearer by returning to the accurate equation which I have proved,

$$s = a + \left(\frac{360 \times 60 \times 60}{15} \cdot \frac{T}{T_o} + \frac{n}{15}\right)t.$$

For since $t\,T = t_o\,T_o$, identically, if t_o denote the true mean solar time, we may write this equation under the form

$$s = a + 24^h\, t_o + \frac{n}{15} . t.$$

If we change n into $n + \delta n$, since

$$(T + \delta T)t_1 = t_o T_o,$$

we have

$$s = a + 24^{h}\, t_{o} + \frac{n + \delta n}{15} \cdot t$$

These equations show at once that if we always suppose the time measured in true mean solar time in the principal term 24^{h}, we simply throw the adjustment of the mean times to the given sidereal times upon the small terms. If we determine our mean solar time, firstly, from the equation

$$s = 24^{h} \cdot t_{o} + \frac{n}{15} \cdot t,$$

secondly, from the equation

$$s = 24^{h} \cdot t_{o} + \frac{n + \delta n}{15} \cdot t',$$

then the times must differ in the proportion of the units $n : n + \delta n$. And if we write these expressions

$$s = a + 24^{h} \cdot t + \frac{n}{15} \cdot t$$

and

$$s = a + 24^{h} \cdot t + \frac{n + \delta n}{15} \cdot t,$$

and simply assume that the mean noons are the same in both cases, so that we can put $t = r =$ number of true mean solar days at true mean noon, which comes to the same thing as writing

$$t = t_{o} \text{ in the principal term,}$$

we must find the values of t as before expressed in terms of units in the proportion of $n : n + \delta n$.

All I have done, therefore, is to simply transfer Le Verrier's expression so as to make it give identical results with those which would have been obtained by the direct use of Bessel's expression unaltered since 1864.

This leads to an important remark and question of Professor Cayley's in the January number of the *Notices* to which I have not yet alluded. Professor Cayley says (p. 85): "Mr. Stone, as I understand him, contends that for the same instant of time the two points (i.e. really the observer's position) are one and the same point." If so, it seems to me that Hansen's formula (or Le Verrier's) must be a mere analytical transformation of Bessel's, and the question would arise, Why was such a change made?" The change made in our expression for the sidereal times *was quite unnecessary*. It was done no doubt because it was supposed that the value of the Sun's mean motion deduced from a discussion of the observations in terms of the adopted unit of time was more accurate than that adopted by Bessel, and it was not known apparently that by adopting this value in the expression for the sidereal time in terms of the solar time we

should necessarily change the adopted unit. But, whatever may have been the reason for the change, there is only one way of estimating its effects, and that is, to transfer the variable t' of Le Verrier (or Hansen), as Professor Cayley practically admits I have done, into that of the t of Bessel. This, at all events, gives correctly the effects of the change upon existing tables. What we want is to separate the discordances between Hansen's tables and observations into two parts—one due to our arbitrary action in 1864, and the other due to errors in the tables themselves. As some serious misconceptions appear to exist upon the point, it should be known that Le Verrier deduced the correction δn to the adopted value n from a discussion of the observations referred to the meridian with the condition

$$s = a + \left(24^{\mathrm{h}} + \frac{n}{15}\right)t + \text{ \&c.};$$

and therefore the value $n + \delta n$, so far as the correction δn was really required, was only true when this relationship existed between s and t; when, therefore, after the construction of the tables, we adopted

$$s = a + \left(24^{\mathrm{h}} + \frac{n + \delta n}{15}\right).t',$$

and assumed at every mean noon, instead of

$$sn = a + \frac{n}{15}.t,$$

that

$$sn = a + \frac{n + \delta n}{15}.t',$$

we actually introduced the error

$$(n + \delta n)t' - nt,$$

or, if t' is supposed to be equal to t, the error

$$\delta n t,$$

or, practically, sensibly doubled the correction δn which Le Verrier thought necessary to reconcile theory and observation with the old unit of time.

With respect to the inexactness of such equations as those connecting s and t: they are used in two ways, in both of which they must be exact; we determine from such equations the mean time from a given sidereal time, or we eliminate by the aid of this equation the variable t, and thus find the tabular positions of the Sun, Moon, and planets on the meridian for direct comparison with our observations.

The following is perhaps the simplest form in which the proof of the change can be put.

If

$$(\alpha)\quad s = a + \left(24^{h} + \frac{n}{15}\right)t$$

is Bessel's expression, I *can* certainly transfer t into t' time as follows. Let

$$nt = (n + \delta n)t',$$

then

$$(\beta)\quad s = a + \frac{n+\delta n}{15}.t' + 24^{h}\left(1 + \frac{\delta n}{n}\right).t',$$

Le Verrier's expression adopted in 1864 was

$$(\gamma)\quad s = a + \frac{n+\delta n}{15}.t' + 24^{h}.t'.$$

The addition of the term

$$24^{h}.\frac{\delta n}{n}.t'$$

makes, therefore (γ) identical with (β), and it can, therefore, be re-transferred identically back into (α). This term is, therefore, the necessary correction to make the mean times deduced from Le Verrier's expression identical with those which would have been obtained from the continued use of Bessel's expression. And since the change from Bessel's expression to Le Verrier's in 1864 was an arbitrary act of astronomers, the discordances thus introduced between Hansen's Lunar Tables and observations are the results of our own action, and have nothing whatever to do with physical causes; and what we require to estimate is the *effect of the change*, and this is what I have done.

I am perfectly at a loss to understand the opposition with which my work has been received. It appears to be considered that there is some absolute physical necessity for the use of the true mean solar day as the unit of time in practical astronomy, although such a use is absolutely impossible for the reasons which I have already given.

Questions respecting Mr. Stone's Theory of Changes in the Mean Solar Day. By Prof. Simon Newcomb.

I have refrained from joining in the discussion of Mr. Stone's views respecting the mean solar day found in the *Monthly Notices* of May last for two reasons. One is that the matter seemed too elementary in its character to be the subject of prolonged discussion. The other was a desire to wait until Mr. Stone should give a statement of his views sufficiently explicit to admit of their correctness being tested by practical application. As the discussion has now been going on for the greater part of a year, the first of these reasons has ceased to be operative. At the same time I note with surprise that Mr. Stone still refrains

from any such statement of his theory as would guide one in making any practical application of it, and therefore ask permission to point out what is wanted. In doing this I do not wish to be considered as taking any ground against his conclusions, but rather as viewing him in the light of a plaintiff who has failed to make a sufficiently explicit statement of his case.

On page 336 of volume xliii. of the *Monthly Notices* Mr. Stone gives a formula for the apparent deviation of the Moon's mean longitude arising from the cause which he claims to act. This expression contains the algebraic symbol t in the product $0''{\cdot}747\ t$, where it is understood that t is the time in years counted from *some* epoch. But neither here nor in any of his subsequent papers, so far as I have found, does he give any general definition of this epoch. Again, on the next page he gives observed corrections of the Moon's mean longitude. But he does not state what his computed corrections for the same dates would be, and thus does not afford the means of determining, even by induction, the epoch of t.

It is apparently implied that the epoch is that at which Le Verrier's tables were introduced into the British *Nautical Almanac*. But here again we must remark that no statement is made to the effect that the introduction of tables into an Ephemeris constitutes a definable epoch in the application of the tables. The same lack of general definition and explicit statement pervades all of Mr. Stone's statements on the subject which I have been able to find. As it is of great interest to all investigators of the lunar theory to know to what extent their work is to be modified should his views be proved correct, I submit the following questions respecting the application of his theory in the spirit of a seeker after information, and with the earnest hope that either Mr. Stone or someone else who has mastered his theory will answer them:—

I. What is the *general* definition of the epoch of t in the expression already alluded to on page 336?

II. What are the theoretical deviations of the Moon's mean longitude arising respectively from the introduction of Le Verrier's tables into the *Nautical Almanac* in 1864, and of Hansen's tables, the mean motion of which is substantially identical with that of Leverrier into the American *Ephemeris* in 1858?

III. On what principle or by what rules are Professor Airy's conclusions respecting the errors of Damoiseau's lunar tables and my conclusions in § 15 of *Researches on the Motion of the Moon* respecting the corrections to Hansen's tables to be modified?

IV. In lieu of this last statement any general formula or precept showing completely by what numbers and on what principles the necessary modifications are to be made, would be acceptable.

Remarks on the Value of the Secular Acceleration of the Moon's Motion derived from Observation. By Prof. Simon Newcomb.

In Thomson's and Tait's "Natural Philosophy," second edition, part ii. p. 415, and Appendix G, p. 503, are found discussions by the authors and by Professor Darwin of the earth's tidal retardation as deduced from various values of the Moon's secular acceleration. The statements of the origin and relations of these different values which accompany these discussions seems to me susceptible of some slight reconstruction, which I am induced to attempt from the belief that any incompleteness which may affect them arises from some defect in my own presentation of the subject, to which the authors refer. I remark first that the value 12″·56, though clearly described in one passage as founded on erroneous theory, which it really is, is spoken of elsewhere as representing the ancient eclipses, and therefore to be regarded as a value derived from observations. In reality, however, this value is not compatible with any observational data whatever. It is true that Hansen's tables, into which a value little different from this is introduced, do represent the ancient eclipses of Thales and Larissa. But this happens through Hansen having used an erroneous mean motion of the Moon at the present epoch. The process of determining the acceleration from ancient eclipses consists essentially in carrying the place of the Moon back with its present mean motion to the epochs of the eclipses, and comparing the outstanding difference between the mean longitude thus computed and that given by the eclipse. Now, in consequence of Hansen's addition of an empirical term of long period to represent the Moon's place from 1750 to 1850, his centennial mean motion is about 29″ in error. When this correction is applied, the acceleration is diminished by 1″·25, thus making it 10″·9 or 11″·3 according to the value which is considered as best representing the data in question.

We may then say that 11″ is the value which best represents the ancient solar eclipses referred to. But this does not exhaust the subject. In my discussion in *Researches on the Motion of the Moon* I showed that this value was incompatible with the great mass of astronomical data, especially the Ptolemaic and Arabian eclipses of the Moon. Since this publication new light has been thrown on the question from two quarters. The tendency of this new light is to establish the smaller value as nearer the truth.

I. As favouring the larger value I cited the incompatibility of the smaller value with a certain eclipse of the Moon supposed to have been observed at Babylon in the year B.C. 382. Now Dr. Oppolzer calls attention to the fact that Hipparchus does not describe this eclipse as actually observed at Babylon, but as being employed as if it had been there observed.

II. Celoria's ingenious research into the eclipse of Theon,

which was total in the Hellespont, seems to show that this was the eclipse of Agathocles; and, if so, the shadow passed north of Sicily. This is compatible only with the smaller value of the acceleration.

If we accept these conclusions or interpretations, nothing is left to support the larger value of the acceleration except the eclipses of Thales and of Larissa, while against it stands an array of evidence which I cannot but regard as many times stronger.

The Parallax of α Tauri. By Prof. Otto Struve.

(*Extract from a Letter to Mr. Gill.*)

With regard to parallaxes the following recent fact will be of some interest for you. During the last few weeks I have been engaged in preparing for publication my second volume of double star measures (vol. x. of the *Observations de Poulkova*). On this occasion I was struck with the remark that about thirty years ago I had collected for *α Tauri* twenty-five measures of each coordinate, and seeing that these measures were favourably distributed in different seasons, I proposed to one of my assistants, M. Shdanow, to investigate if they did not exhibit traces of a sensible parallax. The results of his computations were rather surprising. From the distances he found

$$\pi = +0''.500 \pm 0''.075,$$

and from the positions

$$\pi = +0''.538 \pm 0''.089,$$

the mean being

$$\pi = +0''.516 \pm 0''.057.$$

Such an annual parallax would hardly be expected for a star having so small a proper motion as *α Tauri*, but the agreement of the two values, obtained by two so totally different methods of observation as those of distance and direction, can certainly not leave a doubt as to the existence of a comparatively large parallax, though perhaps, according to the probable error, the real parallax might be somewhat less than that given by my measures.

If you think proper you have my authority to communicate this result to the Royal Astronomical Society.

Pulkowa: 1884, *March* 1.

On an Instance of Change of Personality in Observing Position Angles of Double Stars; and on the Orbit of α Centauri. By A. M. W. Downing, M.A.

In vol. xlvii. of the *Memoirs* of this Society there is a paper by Mr. H. C. Russell containing observations of double stars made at the Sydney Observatory during the years 1870–1881. In this paper will be found a considerable number of observations of *α Centauri*, some of the earlier of which have been used by Dr. Elkin in the determination of the orbit of this binary star contained in his valuable paper, *Ueber die Parallaxe von α Centauri*. But a good many of the Sydney observations made since 1877·5 have not been used in Dr. Elkin's investigation; and these, together with some recent measures by M. Hall, Ellery, and Tebbutt, are available for determining corrections to Elkin's elements in order to obtain an orbit which shall represent the later as well as the earlier observations.

With this object in view the necessary calculations were made, and it was found that, in the mean, the position angles computed from Elkin's elements required corrections of $-7\frac{1}{2}°$ in 1878, and of $-3\frac{1}{4}°$ in 1879–81, to make them agree with the observed position angles. An attempt was then made to modify Elkin's elements so as to make them represent the series of observed angles. After working at the subject for some time, however, I convinced myself that these corrections were erroneous, as I was unable to construct an ellipse which would satisfy the earlier as well as the later measures. As it was the introduction of the Sydney observations made after 1878·0 which caused the great discordance between the computed and observed angles, I determined to exclude them, and then found that the requisite corrections became $-1°·14$ in 1878 and $-1°·26$ in 1879–81. It was evident, then, that the corrections to the assumed elements would be quite small, and I proceeded to find them in the manner now to be described.

From a consideration of the outstanding errors of position angles computed from the assumed elements, the following seven normal places were formed (the angles being measured from the circle of declination passing through the principal star at 1860·0):—

	Epoch.	Normal Angle.	Computed Angle.	Normal Distance.	Computed Distance.
		°	°	″	″
(1)	1827·530	213·34	213·27		
(2)	1835·997	219·34	218·81	16·84	16·69
(3)	1852·177	258·76	258·96	5·50	5·40
(4)	1858·745	335·05	335·12	4·69	4·85
(5)	1875·455	36·61	36·61	5·97	5·92
(6)	1878·380	139·23	139·01	1·96	1·85
(7)	1880·361	187·44	187·70	5·32	5·51

Seven equations of condition were then formed, by means of the usual formula, for the determination of corrections to T, P, and *e*, and solved by the method of least squares, and from the new values of these elements and Elkin's values of λ, γ, and ☊, position angles corresponding to the last six epochs were computed, and from the differences between these and the normal values, and from differential coefficients formed by varying λ, γ, and ☊, equations of condition for determining corrections to these elements were formed, and hence the required corrections found by the method of least squares. The position angles computed from the new elements are given in the column headed "Computed Angle." A correction to Elkin's value of the semi-axis major was then found from the new values of the other elements in conjunction with the normal distances, allowing half weight to the normal distance corresponding to 1878·380. The elements thus found are as follows:—

T	...	...	1875·951
P	...	...	76·222 years
e	...	...	0·51578
λ	...	...	54° 59′·2 (1860·0)
γ	...	...	79 15·2
☊	...	...	25 30·5
a	...	...	17″·33

A detailed comparison of places computed from these elements with the observations made since 1870 is given at the end of this paper. This comparison shows that up to the end of 1877 the Sydney measures of position angles agree, within the limits of error of the observations, with those of other observers, but that in 1878 a remarkable change in the manner of observing occurred, which is continued, though in a less degree, in 1880. This change, which makes Mr. Russell's measures in 1878 (of which there are a considerable number) differ by 5 or 6 degrees from the average of other observers, may perhaps be connected with the change of direction of the line joining the components; the *comes* having passed from the n.f. to the s.f. quadrant towards the end of 1877. It is a useful illustration of one of the many difficulties which beset those who are engaged in investigating the orbits of double stars.

The observed position angles or distances enclosed within brackets in the following table have not been used in determining the elements given above: those measured by Mr. Powell because they were not published at the time when the normal places were formed; the others because of their discordance. It is evident that, thanks to Dr. Elkin's able and thorough investigation, we are now in possession of elements of *α Centauri* which will probably require but small correction in order that they may represent observations made in the future. The corrections deduced in the present paper are of course not entitled to much

weight, as they depend on but few observations; and the subject has been brought forward chiefly with the view of calling attention to the change in the method of observing referred to above.

Epoch.	Observer.	θ_o	$\theta_o-\theta_c$	ρ_o	$\rho_o-\rho_c$
1870·100	Powell	20°45	−0°11	10″24	+0″07
70·608	„	(21·80)	+0·21	(10·09)	−0·48
70·746	Russell	22·32	+0·42	10·46	−0·08
71·050	Powell	(23·01)	+0·52	(9·89)	−0·68
71·310	„	(23·70)	+0·67	(9·80)	−0·59
71·473	Russell	22·91	−0·46	10·23	−0·11
72·468	„	25·33	−0·24	9·73	−0·04
73·331	„	28·07	+0·27	9·50	+0·50
74·150	Ellery	30·50	+0·15	8·00	−0·01
74·468	Russell	30·00	−1·55	7·97	+0·42
74·850	Lindsay	34·17	+0·99		
75·020	Seeliger	34·21	+0·20	6·82	+0·14
76·411	Russell	46·97	+1·52	4·35	+0·27
76·615	Ellery	51·05	+2·55	4·15	+0·47
76·940	„	51·20	−3·87		
77·140	M. Hall	64·40	+3·77	3·30	+0·62
77·248	Ellery	(69·10)	+4·80		
77·510	Russell	72·78	−3·07		
77·556	„	77·25	−1·10	2·11	+0·09
77·580	Gill	80·50	+0·81		
77·593	Russell	81·74	+1·30	1·90	−0·07
77·629	Gill	81·52	−1·08		
77·834	„	97·14	+0·49		
77·888	„	101·04	+0·26		
78·153	Russell	(116·11)	−6·07	1·77	+0·08
78·225	„	(121·66)	−6·11	1·98	+0·26
78·279	„	(127·37)	−4·44	1·77	+0·01
78·380	M. Hall	139·10	+0·22	2·40	+0·55
79·252	Ellery	174·40	+0·83	3·41	+0·14
79·474	Hargrave	(173·55)	−4·17	3·41	−0·30
80·186	Tebbutt	186·70	+0·53	4·97	−0·19
80·351	„	186·40	−1·08	5·31	−0·18
80·444	Russell	(184·98)	−3·18	5·32	−0·36
81·279	Hargrave	(189·88)	−2·81	(5·07)	−2·25
81·545	„	(190·13)	−3·61	7·52	−0·31
1881·655	Tebbutt	193·15	−0·99	7·95	−0·09

Blackheath: 1884, *March* 13.

Note on the Determination of the Planes of the Orbits of Jupiter's Satellites. By A. Marth.

The values of the nodes and inclinations of the orbits of *Jupiter's* satellites which are derived from observed eclipses are liable to a very sensible amount of uncertainty. The eclipses of longest duration give, indeed, the places of the nodes on *Jupiter's* orbit, while those of shortest duration furnish the inclinations. But as the eclipses which occur near the nodes vary little in duration, the ordinary errors of the observations, and, still more, the usual want of observations of successive eclipses at the proper epochs, leave considerable uncertainty in fixing the places of the nodes; and, in the case of the eclipses of shortest duration, if they occur far from the centre, an additional difficulty arises from the uncertainty of the real extent of the sections of the shadow by which the eclipses are produced.

So long as the observations of eclipses were the only trustworthy means available for finding not only the longitudes of the satellites in their orbits, but also the positions of their orbits, the ensuing uncertainty had to be submitted to as unavoidable. But since superior instruments and micrometers have become available, advantage ought to have been taken and ought to be taken to employ them at the proper times for the purpose of procuring those elements which cannot be derived from eclipse observations with the same degree of accuracy. As the apparent motion of the satellites at the beginning and end of nearly central eclipses amounts, during one minute of time, for

Sat. I., II., III., IV.,

to $0'.36, 0''.29, 0''.23, 0''.17,$

when *Jupiter* is nearest to the Earth, or when *Jupiter* is at its mean distance to

$$0''.27, 0''.22, 0''.17, 0''.13,$$

it is easily seen that for determining the longitudes of the satellites in their orbits, micrometrical measurements are not likely to supersede in accuracy trustworthy observations of the times of eclipses, at least when the satellites do not pass too far from the centre of the shadow cone. But the case is different with regard to the determination of the planes of the orbits, especially the fixing of the nodes.

The most promising means for determining the longitudes of the nodes with accuracy will be found in micrometrical measurements of the distances at which the shadows of the satellites near their mid-transits appear from the centre line which bisects *Jupiter's* disc perpendicular to the axis of rotation, if these measurements are made about the times when the planes of the orbits pass through the Sun. That is the case in the present

year, and though the opportunity may not be a very favourable one, since the nodal passages occur when *Jupiter* is too near the Sun to allow proper observations to be made, observers will not soon be so favoured as they were in 1849 or at the end of 1860, when, however, the opportunities were neglected, and they ought to make the best of present circumstances.

The inclinations γ and the ascending nodes Γ of the orbits of the four satellites in reference to the plane of *Jupiter's* equator have been given since 1876, at sufficiently close intervals, in my annual Ephemerides for physical observations of *Jupiter*, the nodes being reckoned from O, the point of the descending node of the planet's equator on its orbit. They depend on data derived from Damoiseau's tables of the satellites, and therefore contain the imperfections of the tables affecting the latitudes of the satellites. The following is a continuation of these values for the present and for the next apparition of the planet:—

	Sat. I.		Sat. II.		Sat. III.		Sat. IV.	
	γ_1	Γ_1	γ_2	Γ_2	γ_3	Γ_3	γ_4	Γ_4
1883.	°	°	°	°	°	°	°	°
July 28	0·0108	330·8	0·4814	332·38	0·1634	256·93	0·3180	330·80
Sept. 26	·0108	328·5	·4820	330·43	·1627	256·47	·3175	330·70
Nov. 25	·0108	326·3	·4826	328·49	·1620	256·03	·3171	330·60
1884.								
Jan. 24	·0108	324·1	·4832	326·54	·1613	255·61	·3168	330·50
Mar. 24	·0108	322·0	·4838	324·60	·1607	255·21	·3166	330·39
May 23	·0109	319·8	·4844	322·66	·1601	254·83	·3166	330·29
July 22	·0110	317·7	·4850	320·72	·1595	254·48	·3166	330·20
Sept. 20	·0111	315·7	·4857	318·79	·1589	254·15	·3167	330·11
Nov. 19	·0111	313·7	·4863	316·86	·1582	253·84	·3169	330·04
1885.								
Jan. 18	·0112	311·9	·4870	314·94	·1575	253·55	·3172	329·98
Mar. 19	·0113	310·1	·4876	313·02	·1568	253·28	·3176	329·93
May 18	·0114	308·4	·4883	311·11	·1561	253·02	·3179	329·91
July 17	0·0115	306·7	0·4889	309·20	0·1553	252·77	0·3183	329·90

The angular distances from the centre of the disc at which, heliocentrically, the apparent orbits of the satellites cross the central meridian will be, according to the assumed data, the following:—

	Sat. I.	Sat. II.	Sat	Sat. IV.
	″	″		″
1884, Mar. 24	−1·09	−2·34	−3·52	−5·70
May 23	−0·60	−1·71	−2·26	−3·72
July 22	−0·11	−1·07	−0·99	−1·74
Sept. 20	+0·37	−0·43	+0·25	+0·22
Nov. 19	+0·84	+0·21	+1·48	+2·16
1885, Jan. 18	+1·30	+0·85	+2·69	+4·05
Mar. 19	+1·74	+1·49	+3·86	+5·90

The conjunction of *Jupiter* with the Sun takes place on August 7. The eclipses of longest duration of the satellites III. and IV. occur on September 4, when *Jupiter* is only 20° distant from the Sun. For testing and correcting the tabular places of the nodes, corresponding observations of the shadows when south and when north of the centre of the disc are required, and observers with superior telescopes and micrometers at their command should endeavour to procure such observations during the latter part of the present and the first half of the next apparition of *Jupiter*.

As regards the determination of the inclinations, it is at present uncertain whether the inclination of the orbit of the *second* satellite can be derived from micrometrical measurements with greater advantage than from eclipses, as the degree of accuracy with which the distances of the shadow can be measured is not yet known. In the case of the *third* satellite, where the number of observed eclipses of shortest duration is much more limited, micrometrical measurements, made at the proper times, will be of essential service for settling the inclination. And in the case of the *fourth* satellite they will be found indispensable if the existing uncertainty is to be effectually removed.

To appreciate this necessity properly, and to get some fair notion of what evidence the observed eclipses of the fourth satellite really furnish for correcting the tabular values of the node and inclination, it may be sufficient, in the absence of an accessible general collection of eclipse observations, to consider the result of those made at Greenwich. The Greenwich eclipse observations, which extend over a longer period than those at any other observatory, have been made with great assiduity, and, I presume, no fair opportunity has been missed; yet, in the course of forty-eight years—from 1836, the year of the publication of Damoiseau's tables, to 1883—the number of eclipses, of which both the beginning and the end have been observed, is only *five*, and of one-sided observations, nine disappearances and sixteen reappearances of the satellite have been secured. None of the five complete observations affords help for correcting the latitude of the satellite. Observations made near the beginning and ending of a cycle of eclipses, when a slight error in latitude shows itself in a telling manner in the observed duration, are very rare. The interesting observation made 1877, August 29, by Todd at Adelaide (*Monthly Notices*, vol. xxxix. p. 16), when the satellite never disappeared, but was reduced to a mere speck of light skirting the margin of the shadow, indicated such an error very strikingly, for the eclipse was, according to the tables, not the first, but the second one of a new cycle, and the tabular duration was seventy-nine minutes. The eclipse of April 4, 1883, the second one of the present cycle of eclipses, showed a similar error, for, according to the observation made with the telescope of the Oxford heliometer by Bellamy (*Monthly Notices*, vol. xliii. p. 446) the satellite disappeared for only six instead of the

predicted eighty-six minutes, while if the "last seen" and "first seen" really refer to corresponding phases, the predicted time of conjunction is nearly correct.

But important as these two observations will ultimately be for determining the extent of the shadow section, they must be supplemented by corresponding observations of eclipses occurring towards the ends of cycles, in order to allow further inferences to be drawn. The only observation near the end of the last cycle which I have yet seen is a one-sided one, a disappearance observed 1880, May 30, by Tebbutt at Windsor, New South Wales (*Astron. Nachr.*, No. 2,541), from which it may be inferred that the tabular longitude of the node requires a sensible positive correction, as the measurements of Bessel and of Schur had already rendered probable. Observers ought to be on the alert early in 1886 to get complete observations of some of the last eclipses in the present cycle.

Observations of Sun-spot Spectra in 1883. By the Rev. S. J. Perry, F.R.S.

The daily routine work at Stonyhurst Observatory has now for some years included a careful drawing of the solar surface on every available occasion, and also a complete measure of the Sun's chromosphere. Since 1882 a spectroscopic examination of Sun-spots has been added to the previous solar work, but attention has been directed exclusively to the red end of the spectrum, from D to B, as the more refrangible portion is being already carefully studied by others. The following paper is a synopsis of the observations made during the past twelve months, and will, it is hoped, present the results in the most simple form for comparison with the work of other observers. In numbering the lines seen widened or darkened in the spots, the figures published in the British Association Report for 1878 have been followed throughout, and the numbers of both Kirchhoff and Ångström are given for the convenience of reference. The instrument used in these observations is an automatic spectroscope of Browning, in which a dispersion of 2, 4, 6, 8, 10, or 12 prisms of 60° can be readily obtained, and a Christie half-prism has been adapted to the original spectroscope, by which the dispersion previously obtained can be trebled. The figures in the columns headed "widened" and "darkened" will not give averages, as clouds often stop an observer in the middle of his scrutiny, and then only a small proportion of the affected lines may have been noted.

TABLE I.

Lines between B *and* D *observed in Sun-spot Spectra.*

No.	Kirchhoff.	Ångström	Number of times Widnd.	Darknd.	Elements.	Remarks.
1	592·7	6867·1		2		B.
2	601·2	43·8		3	Sn	
3	641·0	6717·16	1	3	Ca	
4	654·3	6677·6	2	3	Ba	Chromosphere line.
5	659·3	63·1	2	3	Fe	
6	665·7	43·1	2	3		
7	669·5	33·3	2	3		
8	682·8	6593·3	3	5	Bi, Fe	
9	683·1	92·6				
10	685·3	85·9	2			
11	689·8	74·0	2	3	Fe	
12	690·9	71·4	2			
13	692·1	67·9	2			
14	694·1	62·10	1		H	C twice weakened in spot, once reversed.
15	702·1	43·23	2	3	Fe, Ti	
16	711·4	15·80	3	3		
17		6495·12	11		Fe, Ba, I	
18		94·18				
19	723·7	82·79	6		N	
20	726·7	74·85	6			
21	731·7	61·98	1		Ca, Sb, Cd, Cr	
22	740·9	38·35	2		Ca, Cd	
23	756·9	6399·28	3	2	Fe	
24	759·3	92·87	2	2	Fe, Sb	
25	809·5	6277·09	2		An	
26	809·9	76·32				
27	838·2	6215·67	2		Ti, Fe	
28	838·6	14·30				
29	839·2	12·55				
30	849·7	6190·71	2	3	Fe, Sb, C, E.	
31	859·7	69·59	9	1	As, I	
32	860·2	68·48	1		Ca	
33	861·6	65·62	1		N	
34	863·9	62·69	15		Ca, Rb	
35	864·4	61·40				

No.	Kirchhoff.	Ångström.	Number of times Widnd.	Darknd.	Elements.	Remarks.
36	874·3	40·81	4	4	Ba, Zr	
37	876·5	36·82	13		Fe	
38	877·0	35·82				
39	884·9	21·34	15		Ca, Co	
40	894·9	01·92	15		Ca, Zn, Li	
41	906·1	6077·80	3		Fe, Sb, As	
42	912·1	64·70	3	3	Fe, Ti, Ba	
43	916·3	55·29	2		Fe, Cd, Bi	
44	1002·8	5895·13	12		Na	$D_{,}$ once weakened in spot.
45	1005·0	92·10	2		Ni	
46	1006·8	89·12	12		Na	$D_{,,}$ once weakened in spot.
47	1029·3	56·60		2	Ca, Ni, Mo, Pb, Se, Te	

The Ba line W.L. 6677·6 deserves special notice, as it has been seen several times in the chromosphere during the course of last year, and it is also a marked feature in the spot-spectra.

The chromosphere line K 534·0, which has been occasionally observed in 1883, lies between *a* and B, and is therefore outside that portion of the spot-spectrum which is recorded at this observatory.

In Table II. I have thought it advisable to give the measurements (taken almost entirely from Watts's Index of Spectra) of each element entered in the last column of Table I., in order that the grounds on which their identification rests may be apparent at a glance. In the subjoined list I have included those lines only whose wave-length falls within a tenth-metre of a well-attested measure of some line of an element, or those which are between measures by approved observers, and in estimating the positions of the spot-lines a dispersion of six prisms of 60° has been generally employed, although ten and even twelve have been occasionally tried.

The presence of Ti, Ba, and Ni in sun-spots can scarcely be considered doubtful, although less well-established than that of Fe, Ca, Na, or H. But all the remaining elements decidedly require confirmation; and although some of them rest on very slender foundations, still I have thought it best to adhere strictly to the simple rule of admitting all that satisfy the condition of not differing in their position more than a tenth-metre from the best available measures of the bright lines of the respective elements. From the drawing up of this list it must not be supposed that the evidence adduced would appear to warrant the admission of each named element among solar lines, but it may still be useful to place such close coincidences on record. To avoid needless repetition the authority for each coincident line may be found noted under the special element.

TABLE II.

Elements observed in the Spectra of Sun-spots between B *and* D.

Element.	Line in spot.	Huggins.	Thalén.	Kirchhoff.	Plücker.	Other Authorities.	Coincident Elements.
Fe	6663·1					A.	
	6593·3					B.	Bi
	92·6						
	74·0					A.	
	43·23					A.	Ti
	6495·12	6497				A. G.	Ba
	94·18		6490	6490			I
	6399·28	6400	6399	6399		A.	
	92·87					A.	
	6212·55					A.	
	6190·70	6190	6190	6190		A.	Sb, C, E
	36·82	6138					
	35·82		6136	6136		A.G.	
	6077·80					A.	Sb, As
	64·70		6065	6065		A.	Ti, Ba
	55·29					A.	
Ca	6717·16	6710		6722		A.	
	6461·98	6458	6462	6462		A.G.	Sb, Cd, Cr
	38·35	6434	6438	6438		A.G.	Cd
	6168·48		6168	6167		A.G.	
	62·69	6163					
	61·40		6161	6161		A.G.	Rb
	21·34	6116	6121	6121		A.G.	Co
	01·92		6102	6101		A.G.	Zn, Li
	5856·60	5854	5857	5857		A.G.	Ni, Mo, Pb, Se, Te
Na	5895·13	5895	5895	5895		A.G.	
	89·12	5889	5889	5889		A.G.	
H	6562·10	6562	6562		6562	A.	
Ba	6677·6	6677					
	6495·12	6499	6496	6497		A.G.	Fe
	6140·81		6141	6141		A.G.	Zr
	6064·70	6064	6062	6063			Fe, Ti
Ti	6543·23		6543				Fe
	6214·30		6214			A.	
	6064·70		6064			A.	Fe, Ba
Ni	5892·10		5892	5891		A.	
	5856·60		5856	5856		B.	Ca, Mo, Pb, Se, Te
N	6482·79	6482	6480	6480			

Element.	Line in spot.	Huggins.	Thalén.	Kirchhoff.	Plücker.	Other Authorities.	Coincident Elements.
	6165·62				6165 / 6162 band		
Au	6277·09					B.	
	6276·32	6276	6276	6275			
Sn	6843·8			map			
Bi	6593·3		6599				
	92·6	6590					Fe
	6055·29	6055	6050				Fe, Cd
Sb	6461·98	6461					Ca, Cd, Cr
	6392·87	6392					Fe
	6190·71	6189	6193				Fe, C, E
	6077·80	6076	6078	6080 / 6076			Fe, As
Cd	6461·98	6462	6466	6468 / 6462			Ca, Sb, Cr
	6438·35	6433	6438	6438		Mascart, 6437	Ca
	6055·29	6050	6056				Fe, Bi
As	6169·59			map			I
	6077·80	6078					Fe, Sb
I	6494·18				6494		Fe
	6169·59				6169		As
C	6190·71					Watts, 6190	Fe, Sb, E
Co	6121·34		6121	6121			Ca
Cr	6461·98	6461					Ca, Sb, Cd
E	6190·71		6190				Fe, Sb, C
Li	6101·92	6096	6102	6101			Ca, Zn
Mo	5856·60		5857				Ca, Ni, Pb, Se, Te
Pb	5856·60	5853	5856				Ca, Ni, Mo, Se, Te
Rb	6161·40			map			Ca
Se	5856·60				5856		Ca, Ni, Mo, Pb, Te
Te	5856·60	5854	5856				Ca, Ni, Mo, Pb, Se
Zn	6101·92	6100	6102	6106 / 6099			Ca, Li
Zr	6140·81		6140				Ba

Abbreviations.

A is Angström's Spectre Normal du Soleil.
G is Greenwich Observation of a Sun-spot, 1879.
B is Burton's Catalogue in Brit. Assoc. Report, 1878.

Stonyhurst Observatory:
1884, *Feb.* 21.

Remarks on Mr. Hilger's Illumination of Micrometers with Vacuum Tubes. By Dr. N. de Konkoly.

I read in No. 82 of the *Observatory* that Mr. Hilger exhibited at the January meeting of the Society a micrometer with vacuum tube illumination.

In the year 1875 I made such a micrometer illumination, but soon discontinued its use, because of the unpleasant shocks which I received from the induction current. I had the eyepiece diaphragm made of ebonite, but then received the shocks in the hand on turning the micrometer. The vacuum tube was entirely enclosed in an ivory and ebonite case on account of the induction current producing such unpleasant shocks. In front of the wires was a very small circular vacuum tube which was kindly made for me by my friend, Professor Lengyel, of the University of Buda-Pest. The wires were bright on a dark ground, and the illumination was very good, without any diffused light in the field, but was unpleasant on account of the tremulous light produced by the interruption of the current. I also illuminated the field of view, so as to produce black wires on a bright ground. My telescope being of metal, we still experienced shocks and also a tremulous light. The shocks might have been avoided by making the eyepiece diaphragm and the button of the micrometer-screw of ebonite, but it was not possible to prevent the tremulous light.

I saw a few weeks ago such a micrometer by Mr. Hilger in the observatory of Cardinal Haywald at Kalocsa, but Dr. Braun, the Director of the Observatory, had also rejected the vacuum tube on account of the tremulous light and the shocks which were received every moment.

I have now illuminated all my micrometers with very small incandescent (Swan) lamps, both on the telescope and spectroscopes. I can obtain from M. Müller, of Hamburg, very small Swan lamps of $\frac{1}{4}$-inch diameter, of 1–2 volts electro-motive power, so that they can be put in action by one or two Bunsen cells or a very small accumulator. The light is very steady, and can be easily regulated by applying different resistances to the current, or by employing diaphragms of oiled paper.

I have made use of this arrangement in my observatory for half a year, and think that no one who has tried the method would ever employ vacuum tubes or oil lamps.

I should be pleased to give photographs or drawings of my improved arrangements to anyone who is interested in the subject.

I may add that M. E. de Gothard has illuminated all the micrometers in his private observatory at Herény on my system.

Illumination of Spectroscope Micrometers. By Dr. N. de Konkoly.

I take the liberty of calling the attention of the Society to the employment of small Swan incandescent lamps for illuminating the micrometers of spectroscopes.

I have lamps not larger than a hazel-nut, from Messrs. Greiner and Friedrich's, at Stützerbach, Thüringen, and also the size of a very small pea, just enough for illuminating the micrometer of a spectroscope. I have mounted them in a small tube attached in front of the micrometer, so that on the bottom of the tube is a small commutator. As I turn the commutator button 45° I receive light; as I turn it back all is dark. The lamp illuminates also the division of the micrometer-screw.

I have now arranged all my spectroscopes with electric illumination; three of them have a micrometer-screw, and two a scale, except the large Browning, on which both kinds of micrometer are attached. On this latter are two lamps, the one for the scale, the other for illuminating the wires (bright wires on dark ground) and the division of the micrometer-screw.

I send a photograph for the R.A.S., and am sure that Mr. Browning will not know this instrument in its altered form. There is also attached a second telescope parallel to the observing telescope (not attached in the photograph) for reading from the eye-piece the index of the micrometer division.

I have also arranged my large 10-inch refractor for the electric light. I have a large commutator on the eye-end of the telescope, and this communicates with both verniers of the declination circle, reading from the eye-end, with the illumination apparatus of the micrometer wires and the spectroscopes, and also with the R.A. circle.

The electricity is received through the perforated declination axis by an insulated wire from two or four accumulators, made by myself. They are sufficient for one evening's observation, and can be charged during the day by thirty Meidinger elements, the battery of the Observatory telegraph.

Spectroscopic Observation of the Red-coloured Sky at Sunset, 1884, *January* 9, $5^h\ 20^m$. By Dr. N. de Konkoly.

I used for this observation a Browning pocket-spectroscope, provided with a micrometer, and looked with it at the sky in an altitude between 5° and 10°. I observed in the less refrangible portion of the spectrum (yellow to red) five well-defined bands. The broadest was that towards violet from D, but it was not the darkest. There was a second less refrangible than D, but this was a very indefinite band. At the boundary of the red and orange was a very deep black band, very well defined on both

sides; less refrangible than that in the red was a very broad but pale band; less refrangible than this, nearly at the limit of the spectrum, was visible a very dark band, the blackest of all.

The positions of the bands were as follows:—

1. from 584·6 to 567·5 *
2. „ 603·1 „ 589·2
3. „ 639·5 „ 627·9 *
4. „ 711·0 „ 661·7
5. „ 739·0 „ 711·9

The correction of the zero point of the micrometer was deduced by observation of the bright sodium line in an alcohol flame.

The bands marked * clearly correspond to the lines of atmospheric vapour.

Spectroscopic Observations of Comet Pons-Brooks made at the Observatory O-Gyalla, Hungary. By Dr. N. de Konkoly.

The comet was observed in different phases of its brightness. I made the first observation on September 27 of the past year, using a spectroscope of little dispersion; I found three very ill-defined bright lines in its spectrum.

The second observation was on November 22 in tolerably steady air, at 5^h 50^m O-Gyalla M.T. The spectrum of the comet was pretty bright, and also the three bands, which were not at all difficult to measure. The position of the bands from the mean of three measures were—

I.	II.	III.
561·0	516·5	471·6 mmm.

I estimated the brightness of the bands respectively as 0·3, 1·0, and 0·5, beginning at the red end of the spectrum, the brightness of band II. being taken as unity.

A faint continuous spectrum was visible between wavelengths 608·0 to 447·4.

I next observed the spectrum of the comet on November 29, 6^h 50^m to 7^h 30^m. Band II. in the spectrum was very bright and remarkable. All three bands were pointed at their ends, and badly defined on both sides, a little swollen in the middle and overflowing in the faint continuous spectrum. This was very defined on band II.

The positions of the three bands from the mean of four observations was—

I.	II.	III.
560·9	516·5	471·4 mmm.

The brightness of the bands was estimated as 0·3, 1·0, and

0·5. The limits of the continuous spectrum were 616·2 and 434·6 mmm. wave-lengths.

The astronomer of my observatory, M. Rudolf de Kövesligethy, measured band II. on December 7, and found it a little less refrangible than I did (517·3), but M. Kövesligethy supposed it so himself.

On December 23 we measured the positions of the bands in the large 10-inch refractor. We saw a very well defined continuous spectrum corresponding to the nucleus of the comet. The bands were very well defined on the side towards the red and shaded off on the other side. I measured the edge of band II. five times, and the maxima of the light from all the bands also five times. M. Kövesligethy measured also the edge of band II. and the maximum of III. The results were—

	I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.	Observer.
	560·2	519·5	516·8	471·8	Konkoly
	—	519·0	—	473·2	Kövesligethy.
Brightness:	0·3	—	1·0	0·6	—

The observations were made at $6^h\ 40^m$; the comet was 45° above the horizon.

On December 26 M. Kövesligethy observed the spectrum and found the following positions for the bands:—

	I.	II.	III.
	558·5	517·0	477·4
Intensity:	0·2	1·0	0·6

On December 29, $5^h\ 30^m$, I observed the spectrum of the comet with a large Browning spectroscope provided with 2 fine prisms of 60° refractive angle. The spectrum was very bright, and I was able to count six bright bands. Band I*a* was a very faint one, but very well defined at the edge towards the red and shaded off towards the violet. The second band I. (the first ordinary comet-band) was also well defined towards the red, and showed two maxima of brightness. Band II. was very intense, and showed also two maxima, the edge towards the red being the brighter one. The third cometary band was very broad, and had a sufficiently well defined edge towards the red end; there was also a broad maximum. This band still showed two maxima, but they were somewhat faint.

Band IV. had a light maximum on both sides, much shaded off and badly defined; the ends were pointed as the bands of the spectra of very faint comets. Band V. was broad, but very faint; sharply defined towards the red, and shaded off towards the violet. The slit was open 0·4 mm.

The relative intensity of the bands was estimated to be 0·3,

0·4, 1·0, 0·6, 0·3, 0·1. Clouds prevented me from any series of measures, but I made a good drawing of the spectrum.

M. Kövesligethy made a series of measurements with the other instruments; he found the positions of the three ordinary cometary bands (6-inch refractor).

I. Maxim	II. Edge	II. Maxim	III. Maxim
560·7	518·1	511·6	471·6

On December 30 a set of measures made by M. Kövesgelithy gave—

I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
558·5	514·8	511·6	478·4

and on December 31 the same observer found—

I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
558·5	515 2	511·2	472·7

On 1884, January 1, 6^h 10^m, I observed the spectrum of the comet and made a good drawing of it, but clouds prevented me making exact measures. With the universal spectroscope by Merz, with one direct vision spectroscope of 5 prisms on the 10-inch refractor I again saw the six bands just as I saw them on December 29.

M. Kövesligethy made two series of measures, one with the Merz spectroscope on the 10-inch refractor, the other with a smaller spectroscope on the 6-inch refractor. He found the positions—

	I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
1.	—	—	515·8	470·1
2.	563·0	517·3	511·6	473·3
Intensity:	0·3	—	1·0	0·6

A faint but well-defined continuous spectrum was observed on each occasion.

Observations of Pons' Comet made at the U.S. Naval Observatory, Washington. By W. T. Sampson and Edgar Frisby.

(*Communicated by the Secretaries.*)

	Wash. M.T.	Comet − ✱ Δα	Comet − ✱ Δδ	No. of Com.	Comet. α app.	log. (p×Δ)	Comet. δ app.	log. (p×Δ)	Obs.	Com. Star.	Adopted Mean Positions of Comparison Stars. Star.	α 1884·0.	δ 1884·0.	Authority.
1884.	h m s	m s	′ ″		h m s		° ′ ″					h m s	° ′ ″	
Jan. 2	5 46 50·0	+2 31·64	+ 5 45·0	20,4	21 53 30·44	9·503	+20 47 14·0	0·519	S.	1	1	21 50 59·10	+20 41 17·6	Weisse (2) 21, 1209
2	6 0 5·3	−1 24·42	− 0 56·4	20,4	21 53 34·48	9·531	+20 46 20·0	0·531	S.	2	2	21 54 59·18	+20 47 5·0	,, (2) 21, 1318
3	5 39 53·3	−4 11·65	+ 4 37 6	20,4	22 0 35·66	9·475	+19 7 49·4	0·536	S.	3	3	22 4 47·54	+19 3 1·1	,, (2) 22, 67 & 68
3	7 14 8·9	−0 53·65	+ 6 25·3	20,4	22 1 2·87	9·630	+19 1 4·6	0·618	S.	4	4	22 1 56·77	+18 54 28 6	Ll.+Rüm.+Lam. / 3
5	5 39 40·5	+1 24·73	− 1 49·6	20,4	22 14 39·15	9·450	+15 38 57·5	0·582	S.	5	5	22 13 14·64	+15 40 37·7	Weisse 22, 233
6	7 8 55·7	−1 18·78	− 0 24·8	20,4	22 21 59·81	9·602	+13 44 2·2	0·653	S.	6	6	22 23 18·77	+13 44 18·4	22, 460
7	7 19 46·1	−2 28·32	− 5 47·1	20,4	22 28 52·50	9·608	+11 52 57·3	0·671	S.	7	7	22 31 20·97	+11 58 36·5	22, 626
9	7 29 7·9	−0 30·58	+ 0 44·9	25,5	22 42 14·26	9·607	+ 8 7 15·7	0·698	F.	8	8	22 42 44·97	+ 8 6 24·4	22, 866
3	20 4·6	−2 38·24	− 3 12·8	30,4	23 7 3·89	9·480	+ 0 37 30·3	0·738	S.	9	9	23 9 42·18	+ 0 40 39·8	23, 145
17	7 27 27·8	+3 30·07	− 1 0·1	15.3	23 30 8·96	9·585	− 6 47 39·9	0·774	F.	10	10	23 26 38·99	− 6 46 40·0	23, 502
20	6 2 16·2	+1 11·30	− 4 42·7	25,5	23 45 20·31	9·419	−11 49 34·8	0·818	S.	11	11	23 44 9·12	−11 44 49·9	23, 868
21	6 0 43·1	−0 58·02	+ 9 32·1	20,4	23 50 10·27	9·415	−13 27 3·4	0·827	S.	12	12	23 51 8·40	−13 36 32·5	23, 1014
21	7 10 53·7	+0 35·75	+16 24·8	25.5	23 50 24·10	9·565	−13 31 23·1	0·806	F.	13	13	23 49 48·47	−13 47 44·9	W.+Lamont+Stone / 3
22	7 21 27·7	+0 41·40	+ 1 36·8	20,4	23 55 7·18	9·582	−15 6 33·0	0·807	S.	14	14	23 54 25·91	−15 8 6·2	Arg. Oeltz. S. 23179
25	7 4 26·7	−0 30·76	+ 3 38·6	30,6	0 8 13·98	9·565	−19 30 58·6	0·828	S.	15	15	0 8 44·89	−19 34 31·8	Stone 70
26	6 3 47·0	+1 20·58	+ 1 15·4	20,4	0 12 9·76	9·435	−20 50 6·2	0·858	S.	16	16*	0 10 49·34	−20 51 15·9	Ll. 234
Feb. 1	6 55 21·0	+1 42·42	+ 6 7·0	20,4	0 34 4·88	9·580	−28 9 37·7	0·851	F.	17	17	0 32 22·73	−28 15 36·0	Arg. Oeltz. S. 315

* Baily's reduction of this star is 1′ wrong. Compare also Gould's "Uranometria Argentina."

Observations of Comet Pons-Brooks, made at Vizagapatam, India. By A. V. Nursing Row.

A comet was in our western sky on the 15th instant, sufficiently conspicuous to the naked eye to attract general attention. I believe it is the same comet that was found in September 1883 by Mr. Brooks in the United States.

At Vizagapatam, sidereal time $3^h\ 58^m\ 4^s$, on the 29th instant, the approximate Right Ascension of the comet was $0^h\ 23^m\ 4^s$, Declination, 24° 5′.

Observatory, Daba Gardens, Vizagapatam:
1884, *Jan.* 31.

Observations of Comet Pons-Brooks made on board the Barque "St. Kilda" on a Voyage from Liverpool to Demerara and Home. By Capt. W. L. Rosseter.

(*Communicated by Capt. Toynbee.*)

Date.	Lat. N.	Long. W.	G.M.T.	Star from which Comet is measured.	Angular Dist.
1884.	° ′	° ′	h m s		° ′ ″
Jan. 14	31 22	54 32	10 29 3 P.M.	Capella	90 13 0
15	31 40	53 44	10 32 20	Aldebaran	79 15 0
17	33 43	49 28	9 59 19	Capella	91 11 0
			10 4 29	Aldebaran	77 44 0
			10 8 0	Polaris	95 23 0
			10 58 23	α Arietis	47 10 0
20	33 53	45 32	10 39 31	Aldebaran	75 45 0
			10 41 47	Capella	92 6 0
			10 44 55	α Arietis	48 0 0
21	36 49	43 31	9 24 0	,,	48 21 0
			9 27 53	Aldebaran	75 15 0
			9 30 46	Capella	92 33 0
22	38 3	41 0	9 8 40	α Arietis	48 48 0
			9 11 3	Aldebaran	74 45 0
			9 13 24	Capella	92 54 0
23	39 5	38 30	9 7 7	α Arietis	49 25 0
			9 9 19	Aldebaran	74 27 0
			9 11 28	Capella	93 20 0
			9 13 11	α Cassiopeæ	72 48 0

Sextant Observations of Comet Pons-Brooks made at Monte Video. By the Rev. S. S. O. Morris.

(*Communicated by the Astronomer Royal.*)

20th January, 1884.

H.M.S. "Amethyst" at Maldonado at $9^h\ 30^m$ P.M. { Lat. 34° 57′ 0″ S. / Long. 54 57 30 W.

Distance between Comet and *Aldebaran*	75°	29′	30″
„ „ *Sirius*	99	10	30
„ „ *Fomalhaut*	22	14	30

Resulting position of Comet at $9^h\ 30^m$ P.M. { R.A. 23h 46m 6s / Dec. 12 0 8 S.

21st January, 1884, $8^h\ 56^m$ P.M.

Distance between Comet and *Aldebaran*	75°	1′	0″
„ „ *Sirius*	97	36	0
Position at time of observation	Lat. 34	57	0 S.
	Long. 56	3	0 W.

The New Comet of 1884, *January* 12. By Norman R. Pogson, Government Astronomer at Madras.

A telegram from Kiel Observatory, received on January 16, gave notice of the discovery of a small comet on the 12th, too far south to be observable in Europe, but did not state where or by whom it was found. Circumstances prevented me from looking for the comet until the 21st, when it was speedily recognised. It was round, with a decided condensation towards the centre, and about 90″ in diameter.

Of the following observations, that on January 22 was taken with the eight-inch equatoreal by Troughton & Simms, the six-inch by Lerebours & Secretan being employed for the rest, and a reticle being used in preference to any other form of micrometer available. Moonlight rendered further observations impossible. Corrections for refraction have been duly applied, and all the star positions are from Mr. Stone's Cape Catalogue :—

	Madras Mean Time.	Apparent Place. R.A.	Apparent Place. P.D.	Parallax Factors. R.A.	Parallax Factors. P.D.	Comparisons.
	h m s	h m s	° ′ ″			
Jan. 22	7 25 10	23 10 20·63	131 57 40·9	+9·8455	−0·5925	5 with 12080
23	7 37 46	23 14 50·88	131 59 13·6	+9·8556	−0·5602	1 each with 12062 12080 and 12105
25	7 31 31	23 23 2·98	131 59 6·9	+9·8504	−0·5779	2 with 12123
26	7 27 28	23 26 50·92	131 58 6·6	+9·8469	−0·5884	1 each with 12105 and 12123
26	7 31 35	23 26 52·47	131 58 31·8	+9·8505	−0·5777	2 each with 12222 and 12226
28	7 14 35	23 33 52·49	131 55 46·7	+9·8355	−0·6178	5 each with 12222 and 12226
30	7 14 24	23 40 20·30	131 51 22·0	+9·8362	−0·6145	10 with 12277
31	7 9 59	23 43 18·29	131 49 15·7	+9·8328	−0·6218	3 with 12277

Madras Observatory:
1884, *Feb.* 7.

Variation in the Light of Neptune, from Nov. 29 *to Dec.* 14, 1883.

By Maxwell Hall.

Neptune is favourably situated for observation at present, and the telescope was directed to the planet on Nov. 27 in order to detect, if possible, any trace of colour; there were three small stars in the field of view, *Neptune*, and two stars between the eighth and ninth magnitude which we shall call *a* and *b*. Their configuration was noticed in order to identify the planet next night by its motion. On Nov. 28 the stars were observed again, when it was noticed that the brighter and following star had moved so little that its identity with the planet was considered unsatisfactory, and an accurate measure was taken. Repeating the measure on Nov. 29 the identity of the planet was clearly proved; and the delicate bluish colour observed the two previous nights duly confirmed.

This gives us the following remarkable sequence of colour of the planets, proceeding from the Earth outwards:—*Mars*, reddish; *Jupiter*, a delicate orange; *Saturn*, greenish yellow; *Uranus*, light green; and *Neptune*, slightly blue. *Uranus* is seen tinged with green in this telescope; and so indeed I saw it in the magnificent refractor of the United States Naval Observatory. When in Washington early in April 1880, Professor Asaph Hall courteously asked me to the Observatory one night, and showed me *Uranus*: the greenish hue attracted my attention at once, and the satellites *Titania* and *Oberon* appeared to be as conspicuous in that telescope as *Jupiter's* satellites are in my

own. I would be glad to have this prismatic sequence of colour confirmed, especially with regard to *Neptune*.

Now on the night of Nov. 29 I noticed with surprise that *Neptune* had diminished in brightness and was then no brighter than the two stars *a* and *b*, which had not changed their comparative brightness. These stars have as nearly as possible the same brightness, which has been assumed to be the 8·5 magnitude; and in comparing *Neptune* with the star *a* from time to time, the difference of brightness was *estimated* in parts of a magnitude. The micrometer was applied that night, and assuming that the tabular position of *Neptune* was correct, the common App. R.A. of these two stars was $3^h\ 8^m\ 39^s$ and their App. N. Decl. were $15°\ 49'\ 30''$ and $15°\ 52'\ 28''$ respectively referred to the Equinox at date. Subsequently a diagonal prism was used to change their relative positions; but by this time *Neptune* was somewhat brighter than *a*, and I began to doubt the correctness of the observations made an hour or so earlier.

The following night was cloudy; and it was not until the night of Dec. 2 that the fact of variation was clearly proved.

Neptune was subsequently observed whenever the weather permitted, and his brightness compared with the star *a* in the same field of view. But stormy weather prevailed, the series of observations was continually broken, and certain photometric appliances could not be used; and so it was not until Dec. 15 that the *period* of variation was determined.

But that night the brightness of *Neptune* remained steady; so again on Dec. 16 and 18; that is to say, the variation had suddenly ceased.

The following table contains all the observations. The first three columns require no explanation; the next column gives in some cases the approximate hour of maximum or minimum deduced from observation by assuming a change from max. 7·5 to min. 8·5 in four hours; and by means of this column all the observations have been combined so as to give epochs of max. and min. The corresponding epochs were then computed by assuming a period of $7^h{\cdot}92$, or

h	m	s
7	55	12,

and by taking Nov. $29^d\ 6^h{\cdot}9$ as the first epoch of min., or Dec. $2^d\ 10^h{\cdot}1$ as the first epoch of max., giving preference to the latter, as Kempshot is $5^h\ 11^m$ west of Greenwich, we have as the max. epoch for future reference:—

d	h	
Dec. 2	15·3	G.M.T.

Variation of Neptune, Nov. 29 to Dec. 14, 1883.

Local Mean Time.		*Neptune* greater than *a*.	Approx. Mag.	Epochs of Max. and Min. observed.			Epochs computed.		O−C
d	h			h		d h	d	h	h
Nov. 27	9·0	½	8·0						
28	9·0	½	8·0						
29	7·2	0	8·5		Min.	29 7·2	29	6·9	+0·3
Dec. 1	7·8	¼	8·3		Min.	1 6·8	1	6·4	+0·4
2	7·2	½	8·0	9·2					
	8·2	½–¾	7·9	9·8					
	9·2	¾	7·7	10·2	Max.	2 10·0	2	10·1	−0·1
	10·2	¾	7·6	10·7					
	16·2	¼–½	8·1		Min.	2 14·6	2	14·1	+0·5
4	6·0	½	8·0	8·0					
	7·0	½–¾	7·9	8·6	Max.	4 8·9	4	9·7	−0·8
	9·2	¾	7·7	10·2					
6	5·8	¾	7·7	6·8	Max.	6 7·5	6	9·2	−1·7
	8·2	1	7·5	8·2					
7	6·2	½	8·0	8·2					
	8·8	1	7·5	8·8	Max.	7 8·7	7	8·9	−0·2
	10·0	¾	7·7	9·0					
	14·1	¼	8·3		Min.	7 13·1	7	12·9	+0·2
8	7·5	½–¾	7·9		Max.	8 9·1	8	8·7	+0·4
9	6·2	½	8·0	8·2	Max.	9 8·7	9	8·5	+0·2
	9·2	¾–1	7·6	9·2*					
10	7·2	¾	7·7		Max.	10 8·2	10	8·2	0·0
	14·0	½	8·0		Min.	10 12·0	10	12·2	−0·2
12	6·2	½–¾	7·9	7·8	Max.	12 8·0	12	7·7	+0·3
	8·2	¾–1	7·6	8·2†					
14	9·0	½–¾	7·9		Max.	14 7·4	14	7·3	+0·1
	13·5	½–⅜	8·1		Min.	14 11·9	14	11·2	+0·7
15	6·0	⅜	8·1						
	8·5	¼–½	8·1						

The differences between observation and computation given in the last column are small, excepting indeed on Dec. 6, when a difference of $1^h·7$ occurs; but from the difficulties encountered in making the observations, and the varying effects of cloud, twilight, and moonlight, the work taken as a whole is much better than anticipated before the period was determined.

* 8·7 or 9·7; 9·2 adopted.

† 7·7 or 8·7; 8·2 adopted.

Respecting the cause of this variation, the planet may be assumed to have dark belts on its surface similar to the belts of *Jupiter* and *Saturn*; should these belts break up so as to make one hemisphere darker than the other, the effect of axial rotation would be to produce variations in brightness above and below the mean, as observed; and then the period of variation would simply be the period of axial rotation.

Should such variations be noticed again, we could hardly assume that the positions of maximum and minimum brilliancy on the surface of the planet had remained unchanged; and therefore we could not use the preceding epochs for a more accurate determination of the period, without the greatest care at least.

It may be further remarked that the period is such that if the planet were observed night after night at nearly the same hour, the variations would escape notice under ordinary circumstances; and this allows us to assume that variations have occurred before, and will occur again; so that a watch should be kept upon the planet at hours differing from night to night.

Kempshot Observatory, Jamaica:
1883, *Dec.* 22.

Observations of the Occultation of Venus by the Moon, made at the Royal Observatory, Greenwich, 1884, February 29.

(*Communicated by the Astronomer Royal.*)

	Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Obs.
					h m s	
(*a*)	Disapp. First contact	Simms Eq.	178	Dark	3 2 50·12	W.C.
(*b*)	„	S.E. Eq.	220	„	3 2 51·19	M.
(*c*)	Last contact	Simms Eq.	178	„	3 3 18·12	W.C.
(*d*)	„	S.E. Eq.	220	„	3 3 13·53	M.

Notes.

(*a*) The time for first contact is probably accurate within one or two seconds. A decided piece had been cut off the *s.p.* (bright) limb of *Venus* at 3^s after the time noted, when I looked at the chronometer to verify the count of seconds.

(*c*) The time for last contact is, I think, accurate within 1^s. The time noted is that at which the last minute point of light had certainly disappeared—it was distinctly seen up to 1^s before the time noted, and all was over 1^s after, when I looked at the chronometer to verify the count. The Moon's dark limb was not seen, but the bright limb was easily visible, though very much fainter than *Venus*.

Clouds covered the sky till within 2^m or 3^m of the time of first contact, leaving only just time enough to get the planet into the field. W.C.

(*b*) Immediately before first contact, the chronometer was looked at in order to begin a new count, and on looking into the telescope again it was

immediately noticed that there was a slight flattening of the limb of *Venus*. The dark limb of the Moon came on so fast that it was believed that the contact had been seen from its very commencement, but the time may possibly have been a little late. As the count had only just been commenced, there was no possibility of any error having been made in counting the beats.

(*d*) The last contact was observed at the thirty-first beat after $1^h\ 38^m\ 0^s{\cdot}0$ by the chronometer. The chronometer beats five beats to 2^s; resulting chronometer time is therefore $1^h\ 38^m\ 12^s{\cdot}4$. The chronometer was referred to before $1^h\ 38^m\ 16^s{\cdot}0$ after the disappearance of the last vestige of the planet. Image of planet at moment of occultation, steady; dark limb of Moon sharp and straight. M.

The clear aperture of the object-glass of the S.E. Equatorial is $12\frac{3}{4}$ inches, of the Simms Equatorial 6 inches.

The initials W.C. and M. are those of Mr. Christie and Mr. Maunder.

Royal Observatory, Greenwich:
1884, *March* 14.

Occultations Observed at Forest Lodge, Maresfield.
By Captain William Noble.

Occultation of Venus, 1884, *February* 29.

Clouds were driving across the sky up to the instant of dis appearance, which phenomenon was not observed. At reappearance, however, the first glimpse of the planet's limb was caught at $2^h\ 53^m\ 48^s$ local sidereal time $= 4^h\ 18^m\ 15^s{\cdot}0$ local mean time, the gibbous limb of *Venus* separating sharply from that of the Moon at $2^h\ 54^m\ 9^s$ L. S. T. $= 4^h\ 18^m\ 36^s{\cdot}5$ L. M. T. This last determination may be regarded as one of considerable accuracy.

By some curious and inexplicable omission, no mention whatever of this interesting and important phenomenon is to be found in the *Nautical Almanac*.

Occultation of λ *Geminorum*, 1884, *March* 6.

The star disappeared instantaneously at the Moon's dark limb at $9^h\ 10^m\ 15^s{\cdot}4$ L. S. T. $= 10^h\ 10^m\ 5^s{\cdot}7$ L. M. T., and reappeared at the bright limb of the Moon at (very approximately) $10^h\ 12^m\ 30^s{\cdot}1$ L. S. T. $= 11^h\ 12^m\ 10^s{\cdot}2$ L. M. T.

Occultation of κ *Cancri*, 1884, *March* 8.

The star disappeared instantaneously at the Moon's dark limb at $9^h\ 27^m\ 10^s{\cdot}4$ L. S. T. $= 10^h\ 19^m\ 6^s{\cdot}1$ L. M. T. The reappearance was not observed.

In each case a power of 154 was employed on my 4·2-inch

Ross Equatoreal. From the times given above, 17·8 seconds must be subtracted to obtain the corresponding times at Greenwich, that being the longitude of my Observatory East. Its latitude is 51° 0′ 56″·3 North.

Forest Lodge, Maresfield, Uckfield:
1884, *March* 12.

Note.—The Superintendent of the *Nautical Almanac* has informed the President that the calculation of the occultation of *Venus* for Greenwich was made in the usual course. It happened to be upon a form by itself, and was accidentally overlooked in making up the copy of occultations visible at Greenwich for the press. The omission subsequently escaped detection.

Note on the Transit of the IVth Satellite of Jupiter, 1884, *March* 12. By Edmund J. Spitta.

Whilst looking at *Jupiter* on March 12 at 9 P.M. I observed what I took to be a very well-defined shadow. On referring to the *Nautical Almanac* I found it was the IVth satellite itself —pitch black, transiting the disc of its primary, having accomplished about one-third of its course.

I left the telescope, and returning at 11 P.M. again saw the satellite—now more than half way across—as well defined and *black* as before. I note this because I believe it unusual for satellite IV to appear *black*, the colour more often being chocolate or brown. The spot seemed lifted off the planet.

At 11.45, as the time of egress was approaching, the satellite began to lose both its colour and definition; whilst at 11·50 it was still less apparent, being hardly visible five minutes later. I now fancied in the next few seconds I saw the satellite illuminated on the disc, but as the air at the moment was very unsteady I cannot be certain. At 12 o'clock, during a wave of magnificent definition, to my surprise I distinctly re-saw the satellite, almost as black as before, *quite close to the limb*, which I believe in this situation is very unusual. During the next seven minutes it was entirely lost to view, but at 12.7, one minute before the egress, it burst into unmistakable brilliancy.

I did not observe the ingress, but I am informed by one who did, that it was bright until lost in the dazzling splendour of the planet.

Ivy House, Clapham Common:
1884, *March* 13.

Phenomena of Jupiter's Satellites observed at Stonyhurst in 1883. By the Rev. S. J. Perry, F.R.S.

		Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
1883, Jan.	3	II.	Tr. E. Internal contact	5 35 20·9		J.R.	Definition fair. Hazé.
			Bisection	37 48·0			
			External contact	40 7·3			
	20	I.	Oc. D. External contact	5 40 19·8		J.R.	Definition poor. Haze.
			Last seen	43 45·3			
Feb.	11	I.	Tr. I. External contact	8 22 44·0		W.McK.	Good.
			Bisection	26 38·0			
			Internal contact	29 47·5			
	11	I.	Tr. E. Internal contact	10 39 52·9		W.McK.	Difficult. Haze.
			Bisection	43 47·9			
			External contact	47 0·4			
	12	I.	Ec. R. First seen	8 59 14·7	$-0^m\ 30^s\cdot3$	W.McK.	Very satisfactory.
			Half light	9 0 36·1			
			Full light	1 55·9			
	16	III.	Oc. R. First seen	7 1 23·6		W.McK.	Good. Haze.
			Bisection	4 36·1			
			External contact	7 4·1			

	Satellite.	Phenomena.	G.M.T., h m s	Corr. of N.A.	Observer.	Remarks.
Feb. 16	III.	Ec. D. Losing light	9 6 52·6		W.McK.	Excellent.
		Half light	9 24·6			
		Last seen	15 54·6	+ 2 34·6		
16	II.	Oc. D. External contact	11 53 6·1		W.McK.	Haze at beginning. Very good at end.
		Bisection	55 45·1			
		Last seen	59 33·6			
16	III.	Ec. R. First seen	11 53 25·6	− 47·4	W.McK.	„ „
		Full light	58 25·1			
18	II.	Tr. I. External contact	6 24 46·5		W.McK.	Very good.
		Bisection	26 46·0			
		Internal contact	28 12·7			
Mar. 4	II.	Tr. I. External contact	11 27 55·0		W.C.	Definition very poor.
		Bisection	29 43·1			
		Internal contact	31 14·5			
5	I.	Oc. D. External contact	11 11 31·2		W.C.	Definition fair.
		Bisection	13 15·4			
		Last seen	14 46·7			
7	I.	Ec. R. First seen	9 15 48·5	− 8·5	S.P.	Very good.
		Half light	17 16·2			
		Full light	18 36·0			

	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
Mar. 12	I.	Oc. D. Bisection	13 8 38·4		W.C.	Definition very bad. Haze.
		Last seen	9 50·4			
13	III.	Tr. E. External contact	8 54 35·1		J.R.	Definition good. Clouds passing.
14	I.	Oc. D. External contact	7 36 54·0		W.C.	Good.
		Bisection	38 50·5			
		Last seen	40 26·3			
14	I.	Ec. R. First seen	11 11 46·9	+ 0·9	W.C.	Good.
		Half light	12 52·4			
		Full light	17 12·9			
31	II.	Ec. R. First seen	8 35 27·9	+ 13·9	W.C.	Good.
		Half light	37 11·8			
		Full light	38 46·8			
31	III.	Ec. D. Fading	9 12 28·3		W.C.	Very good.
		Half light	15 21·6			
		Last seen	19 1·2	+ 2 20·2		
31	III.	Ec. R. First seen	12 3 33·1	− 1 15·9	W.C.	Good.
		Half light	6 46·7			
		Full light	9 41·7			

		Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
April	4	IV.	Ec. D. Fading	9 29 22·7		S.P.	Very unsatisfactory. The thin cloud over *Jupiter* seemed to absorb the light of IV. more than that of I., II., or III.
			Last seen	53 0·4	+19 6·4		
	4	IV.	Ec. R. First seen	10 45 21·9	−14 26·1	S.P.	Good. Cloudy half a minute later, when IV. still very faint.
	6	I.	Oc. D. External contact	7 56 33·8		W.C.	Fair.
			Bisection	58 3·8			
			Last seen	8 0 9·3			
	6	I.	Ec. R. First seen	11 28 22·8	+ 19·8	W.C.	Definition very bad.
			Half light	29 38·7			
			Full light	30 29·2			
	7	III.	Oc. D. External contact	8 12 49·5		W.C.	Definition very good. Hazy.
			Bisection	17 24·6			
			Last seen	21 29·9			
	12	IV.	Tr. I. External contact	7 23 44·1		S.P.	Good. Twilight strong.
			Bisection	33 42·4			
			Internal contact	47 32·1			
	12	IV.	Tr. E. Internal contact	8 37 54·0		S.P.	Limb fair.
			Bisection	45 40·5			Observation difficult.
			External contact	55 0·8			

	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
Apr. 15	I.	Ec. R. First seen	7 51 32·4	− 1 0·6	W.C.	Passing clouds. Planet very faint at times.
		Half light	53 7·9			
		Full light	54 44·9			
May 6	III.	Ec. R. First seen	8 10 32·9	− 1 15·1	S.P.	Good. Twilight strong.
		Half light	13 2·4			
		Full light	15 25·2			
16	II.	Oc. D. External contact	9 4 1·9		W.C.	Planet very unsteady.
		Bisection	5 18·4			
		Last seen	11 22·7			
Oct. 5	IV.	Ec. D. Fading	15 41 7·2		W.C.	Excellent.
		Half light	45 14·2			
		Last seen	49 20·6	+ 8 6·6		
5	I.	Ec. D. Fading	16 18 32·1		W.C.	Excellent.
		Half light	19 48·6			
		Last seen	21 1·4	+ 48·4		

Stonyhurst Observatory:
1884, *Jan.* 23.

Observations of Occultations of Stars by the Moon and of Phenomena
Francisco, and at Table Mount Station,

(*Communicated by*

Occultations of Stars

Observed at the *Davidson Observatory*, San

Latitude 37° 47′ 24″·1. Longitude 122° 25′ 37″·6 W.

No.	Date.	Star.	Estim. Mag.	Phase, Im. or Em.	Obser-ver.	Telescope. Aper.	Telescope. Power.	Chronometer.
	1881.					inches.	diams.	
1	June 11	?		Im.	G.D.	6·4	?	Sid. 223
2		c^2 Ophiuchi		,,	,,	6·4	?	,,
	1882.							
3	Mar. 23	ω^2 Tauri	$5\frac{1}{2}$	Im.	C.B.H.	3·0	105	Sid. 223
4	28	A′ Cancri	6	,,	,,	6·4	30	,,
5	29	ω Leonis	$5\frac{1}{2}$	,,	,,	6·4	30	,,
6	Apr. 19	Jupiter		,,	J.S.L.	6·4	120	,,
7		,,		,,	J.J.G.	3·0	105	Sid. 207
8		,,		,,	,,	,,		,,
9	20	?	8	,,	C.B.H.	3·0	105	Sid. 223
10	28	ϵ Leonis	$4\frac{1}{2}$	,,	F.M.	6·4	?	,,
11	May 19	?	7·5–8	,,	C.B.H.	6·4	95	,,
12		?	7·5–8	,,	,,	,,		,,
13		?	9–10	,,	,,	,,		,,
14		?	$8\frac{1}{2}$	,,	,,	,,		,,
15		?	8	,,	,,	,,		,,
16		?	7 5–8	,,	,,	,,		,,
17		?	8·5	,,	,,	,,		,,
18		?	8–8·5	,,	,,	,,		,,
19	June 19	B.A.C. 3202?	7	,,	,,	,,		Sid. 207
20	20	19 Sextantis	$6\frac{1}{2}$	,,	,,	3·0	105	,,
21	21	55 Leonis	6	,,	F.W.	6·4	30	,,
22		,,	6	,,	C.B.H.	3·0	105	M. 7,4969
23		57 Leonis	7	,,	F.W.	6·4	30	Sid. 207
24		,,	7	,,	C.B.H.	3·0	105	M. 7,4969
25	Oct. 17	?	$7\frac{1}{2}$	,,	,,	6·4	95	M. 7,2256
26		?	9–10					
27		?	8					
28		?	$7\frac{1}{2}$–8					
29		?	$7\frac{1}{2}$–8					
30		?	8–$8\frac{1}{2}$					
31		?	8					

There is no explanation of the

of Jupiter's Satellites, made at Davidson Observatory, San California. By George Davidson.

the Secretaries.)

by the Moon.

Francisco, from 1881, June 11, to 1882, Dec. 31.

(Telegraphically determined). Elevation above the sea 378 feet.

Time as recorded.	Chron. correction.	Local Mean Time.	Local Sid. Time.	Remarks, etc.
h m s	h m s	h m s	h m s	
15 30 1·3	−0 0 16	10 7 8·4	15 29 59·7	During total lunar eclipse.
15 52 28·3	−0 0 1·6	10 29 31·7	15 52 26·7	
7 14 13·9	−0 1 21·8	7 6 52·2	7 12 52·1	Good; instantaneous.
10 16 28·1	−0 1 24·2	9 48 54·6	10 15 3·9	Very good; instantaneous.
9 26 23·0	−0 1 25·2	8 55 0·9	9 24 57·8	Good; disappearance gradual
10 39 31·0	−0 1 50·2	8 44 57·9	10 37 40·8	Entire disappearance.
10 40 22·5	−0 3 8·4	8 44 31·2	10 37 14·1	Contact.
10 40 47·0	−0 3 8·4	8 44 55·7	10 37 38·6	Entire disappearance.
10 57 21 ± 2	−0 1 51·4	8 58 47·8	10 55 29 6	About 11′ N. of centre; very faint and doubtful.
13 27 41·2	−0 1 59·7	10 57 7·8	13 25 41·5	Atmosphere very unsteady.
11 58 45·9	−0 2 25·7	8 5 27·1	11 56 20·2	Very good,* about 11′ N. of centre.
12 4 29 7	,,	8 11 10·0	12 2 4·0	,, 11′ S. ,,
12 6 52	,,	8 13 31·8	12 4 26·3 ± 1ˢ·5.	13′ N. ,,
12 14 30	,,	8 21 8·6	12 12 4·3 ± 0ˢ·5.	9′ N. ,,
12 26 56·5	,,	8 33 33·1	12 24 30·8	Good. 6′ N. ,,
12 37 6 4	,,	8 43 41·3	12 34 40·7	Very good. 1′ S. ,,
12 38 45·0	,,	8 45 19·6	12 36 19·3 ± 1ˢ·0.	1′ S. ,,
12 48 30 7	,,	8 55 3·7	12 46 5·0	Good. 4′ N. ,,
14 31 18·4	−0 7 3·1	8 31 4·7	14 24 15·3	Pretty good.
15 18 41·8	−0 7 6·8	9 14 20·7	15 11 35·0 ± 0ˢ·1	
14 6 30·2	−0 7 10·5	7 58 21·3	13 59 19·7	F.W. satisfied sec. should be 30ˢ·2.
5 13 34·4	+2 44 47·1	7 58 21·5	13 59 19·9	
14 42 2·8	−0 7 10·6	8 33 48·1	14 34 52·2	
5 49 1·1	+2 44 47·2	8 33 48·3	14 34 52·4	
5 28 44·9	+ 36 24·2	6 5 9·1	19 51 2·3	* 8′ N. of centre, approx.
5 33 32	24·2	6 9 56·2	19 55 50·1	Very doubtful 3′ S. ,,
5 40 26·8	24·2	6 16 51·0	20 2 46·1	6′ N. ,,
6 26 57·9	24·1	7 3 22·0	20 49 24·7	6′ N. ,,
6 29 57·8	24·1	7 6 21·9	20 52 25·1	11′ N. ,,
6 39 45	24·1	7 16 9·1	21 2 13·9 ± 0ˢ·5	15′ N. ,,
7 2 42·4	24·1	7 39 6·5	21 25 15·1	3′ N. ,,

Observer's initials in the MS.

No.	Date. 1882.	Star.	Estim. Mag.	Phase, Im. or Em.	Obser. ver.	Telescope. Aper. inches.	Telescope. Power. diams.	Chronometer.
32	Oct. 17	?	8–8½					
33		15 Sagittarii	5					
34		16 Sagittarii	6					
35		?	8½–9					
36		?	8½					
37	18	?	8	Im.	C.B.H.	6·4	55	M. 7,2256
38		?	8½					
39		?	8½					
40		?	7					
41		?	8½					
42	Nov. 19	9 Piscium	6	Im.	C.B.H.	6·4	95	M. 7,4969
43	Dec. 18	B.A.C. 286	6½	„	„	6·4	?	Sid. 223

Notes.

1 and 2.—Chronometer 223 fast, May 23 = 15ˢ·6, June 14 = 1ˢ·63, June 23 = 1ˢ·72, rate and error derived above from last two observations.

11 to 18.—Chron. error not satisfactory; depends on rate of 223 for a twelve days interval (but from Jan. to July 1882 the daily rate of this chronometer only ranged from 1ˢ·0 to 1ˢ·2). On this day, as well as Oct. 17 and 18, the points of disappearance are taken from rough-sketch made at the time.

Phenomena of the

Observed at the *Davidson Observatory*, San

	Date. 1882.	Satellite.	Phenomenon.	Phase of Phenomenon.	Obser-ver.	Instrument. Aper. inches.	Instrument. Power diams.
1 (*a*)	Feb. 21	I.	Ec. reap.	First seen	C.B.H.	6·4	250
(*b*)				Half br.			
(*c*)				Full			
	27	I.	Tr. ingr.	Internal contact	G.D.		250
3	27	I.	Sh. ingr.	„	„		
4	Mar. 5	III.	Tr. egr.	External contact	„		250
	7	I.	Oc. dis.	Entirely covered	C.B.H.		150
6 (*a*)	16	I.	Ec. reap.	First seen	„		?
(*b*)				Half br.			
(*c*)				Full br.			

Remarks.

1. (*a*) Well observed. (*b*) and (*c*) very uncertain. Air somewhat unsteady.
2. Air steady.
3. Air rather unsteady.
4. Atmosphere much disturbed.
5. Atmosphere very much disturbed.

Time as recorded.	Chron. correction.	Local Mean Time.	Local Sid. Time.		Remarks, etc.
h m s	h m s	h m s	h m s		
7 24 8·5	24·1	8 0 32·6	21 46 44·7 ±	0ˢ·5	8′ N. of centre, approx.
7 46 5·2	24·0	8 22 29·2	22 8 44·9		1′ S. ,,
7 50 1·3	24·0	8 26 25·3	22 12 41·7		13′ N. ,,
7 53 47·8	24·0	8 30 11·8	22 16 28·8		?′
7 55 23·0	24·0	8 31 47·0	22 18 4·2 ±		?′
5 37 25·4	+0 36 22·7	6 13 48·1	20 3 39·2		✶ ½′ S. of centre, approx.
6 3 31·5	22·6	6 39 54·1	20 29 49·5 ±	0ˢ·2	6′ N. ,,
6 11 29·7	22·6	6 47 52·3	20 37 49·0		5′ N. ,,
6 18 25·9	22·6	6 54 48·5	20 44 46·4		2′ N. ,,
6 30 38·4	22·6	7 7 1·0	20 57 0·9		7′ N. ,,
3 53 45·9	+3 51 59·4	7 45 45·3	23 42 1·3		
1 36 49·4	− 1 47·34	7 44 26·16	1 35 2·6		Well observed.

Notes—continued.

42.—Clock error unsatisfactory. It depends on rate of Sid. clock 1449 from Oct. 28, but clock has been running steadily all year. Rate from June 25 to Oct. 17 = 3ˢ·8 losing daily, Oct. 17 to 18 = 3ˢ·89; Oct. 18 to 27 = 3ˢ·89, and from Oct. 27 to 28 = 3ˢ·93. The star disappeared very close to N. horn and disappearance was to eye of observer gradual: it seemed to "glide in" during about 0ˢ·3. Reappearance not well obtained; first seen at 4ʰ 16ᵐ 36ˢ by chronometer, and estimated time of contact = 4ʰ 16ᵐ·4. On all other dates chronometer error was carefully observed for.

Satellites of Jupiter.

Francisco, during the year 1882.

Chron.	Correction to Chron.	Chron. Time as noted.	Corrd. Local Mean Time.	Time by Amer. Eph.	App. Error of Amer. Eph.
	s	h m s	h m s	h m s	
Sid. 223	−53·5	5 6 4·3	6 57 29·0		
,,	,,	6 30	57 55	6 57 34·4	− 5·4
,,	,,	7 18	58 43		
,,	−59·0	6 46 8·9	8 13 36	8 7 5	
,,	,,	7 59 45	9 27 0	9 23 5	
,,	−66	7 21 48·2	8 25 27	8 23 5	
,,	−67	6 30 50	7 26 45	7 26 5	
,,	−75·8	6 53 35·5	7 13 54·5		
,,	,,	54 45	15 15	7 13 44·3	+10·2
,,	,,	56 0	16 30		

Remarks—continued.

6. (*a*) May be as much as 5ˢ late; sparkle of the satellite seen almost immediately after returning eye to telescope after absence of less than 5ˢ. (*b*) and (*c*) Satellite III. close by for comparison.

Occultations of Stars

Observed at Table Mountain Triangle, Marin Co., California, by Main Triangulation

Position of Triangulation Station { 37° 55′ 19″·3 / 8ʰ 10ᵐ 23ˢ·5

No.	Date. 1882.	Star.	Estim. Mag.	Phase, Im. or Em.	Observer.	Telescope. Aper. inches.	Telescope. Power. diams.	Chronometer.
1	Aug. 22	B.A.C. 5663?	7	Im.	G.D.	2·0	50?	M. 7,5038
2		,,		,,	C.B.H.	3·0	105	,,
3		?	9	,,	,,	3·0	105	,,
4	24	?	7	,,	,,	3·0	105	,,
5	Sept. 16	B.A.C. 4896, 4900?	7	,,	G.D.	3·0	105	,,
6		,,		,,	J.F.P.	2·0	50	Sid. 223
7		,,		,,	C.B.H.	2·0	50	,,
8	21	?	9	,,	G.D.	3·0	105	M. 7,5038
9		B.A.C. 5920?	7·5	,,	C.B.H.	3·0	105	,,
10	22	B.A.C. 7016?	7	,,	,,	3·0	105	,,

Notes.

1. Star first disappeared behind horizontal cross threads, and immerged while thus hidden; noted distance as above ± 0ˢ·05.

1 and 2. The chronometer error depends on set of 24 equal altitudes of Sun on this day, with rate to Aug. 24, &c. Probable error = ± 0ˢ·1.

4. The chronometer error depends on trial set of ✶ transits by Sub.-Asst. J. F. Pratt.

5. For error of chronometer in this and subsequent observations, see records at Table

by the Moon.

Party, U.S. Coast and Geodetic Survey, in charge of Prof. George Davidson, Assistant.

Approximate elevation above the sea 2,592 feet.

Time as recorded.	Correction to Chron.	Local Mean Time.	Local Sid. Time.	Remarks, etc.
h m s	m s	h m s	h m s	s
8 15 14·5	+2 28·5	8 17 43·0	18 23 11·1	±0ˢ·5. See note below.
8 15 15·1	+2 28·5	8 17 43·6	18 23 11·7	Pretty good.
9 11 35	+2 28·6	9 14 3·6	19 19 41·0	±1ˢ·0. Very faint and doubtful.
9 5 19·0	+2 34·5	9 7 53·5	19 21 22·9	Dark limb invisible.
6 58 56·2	+3 59·6	7 2 55·8	18 46 45·4	Fog cloud making star and limb of moon very faint, and observation difficult.
18 46 24·5	+0 20·0	7 2 54·9	18 46 44·5	
18 46 24·3	+0 20·0	7 2 54·7	18 46 44·3	
9 14 30·5	+4 9·1	9 18 39·6	21 22 34·3	Very faint; doubtful.
9 45 57·4	+4 9·1	9 50 6·5	21 54 6·4	Good.
8 9 37·3	+4 13·7	8 13 51·0	20 21 31·6	Very good.

Notes—continued.

Mountain Triangle, 1882, time and latitude comparisons, Sept. 16, P.M.—

	h m s	h m s
S. 223 =	18 51 38·0	18 54 38·5
M. 5038 =	7 4 8·0	7 7 8·0

Other comparisons in record mentioned above.

In all except No. 4, the dark border of the moon was visible.

U.S. Coast and Geodetic Survey Office,
Washington: 1884, *Jan.* 22.